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Discussions

Interactive comment on "An Extended Kriging method to interpolate soil moisture data measured by wireless sensor network" *by* Jialin Zhang et al.

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Comments:

This paper introduces an interesting alternative model for prediction of a spatial variable by kriging. However, it does not reflect adequately on the motivation of the model, or explore its implications. There is therefore no real justification for the selection of the model, which strikes me as very implausible. Furthermore, the model is not implemented correctly. I expand on these statements below.

ANS:

Many thanks to the very long comments from Referee#2. We can see that Referee#2 is a real expert in geostatistics, and have made deep thinking to the proposed algo-

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rithm. All his comments are pertinent to the proposed method and very illuminating to us. We (the authors), however, are researchers in the field of remote sensing applications. We are single-mindedly trying to use remote sensing data to improve the estimation of soil moisture distribution. The emergence of WSN, as a new ground observation technique, brings not only potential benefit, but also the challenges to data quality control and the method on using these data. As we have known, soil moisture is an extremely heterogeneous environmental variable, very hard to be satisfactorily retrieved by remote sensing alone, or interpolated by Cokriging or even Kriging with External Drift algorithm with ground observations alone. The algorithm proposed in this manuscript is designed to suit the satellite remote sensing data and WSN observations, and it shows promising effect to some extent. Although we are quite aware that the proposed algorithm is not optimal and not even rigorous, we still wish to introduce it to the community, so that other experts, such as the anonymous referees, can help us to improve it. So, thanks very much to the referees and editors of HESS.

Comments:

First, there is no reason not to extend the geostatistical model from 1, 2 or 3 spatial dimensions to a space of higher dimensions. This is done, after all, in space-time geostatistics. However, if one is to treat some covariate as defining an additional dimension then, if one is assuming intrinsic stationarity in the new space, as one must for this extension of ordinary kriging, then this implies that there is no systematic relationship between the target variable and the covariate.

ANS:

It is true that the statistical correlation between soil moisture and the spectral variables is very weak. Following the suggestions of Referee#1, we have added some analysis on the soil moisture and the spectral variables into the manuscript (see Sect. 3.2 in the new manuscript). Figure 3 indicates that neither NDVI nor albedo has stable correlation with soil moisture (correlation coefficients less than 0.5). Land surface temperature has a better correlation with soil moisture (Fig. 4), but it is not an easily available data source. I am not saying that there is no systematic relationship at all, but the relationship is too much complicated and unstable to use.

Comments:

In terms of the definition of the intrinsic hypothesis of stationarity E[Z(s)-Z(s+h)]=0 where Z(s) defines our random variable expressed as a random function of the covariate. In words, one is envisaging a situation in which a plot of the observations z against the corresponding values of the covariate s would not show any systematic trend but rather a random fluctuation, exhibiting some degree of correlation. This seems a rather implausible model to me. However, it might be worth considering.

ANS:

Actually, we should admit that more tests on the stationarity of soil moisture in the combined spatial and spectral space are needed. However, our data source is limited: the WSN observation nodes are scarce, and their data quality is not stable enough. As to the simulated data, i.e., the soil moisture map retrieved from airborne hyperspectral image, it is not the real soil moisture and its relation to the spectral variables could be misleading. Therefore, we can only ASSUME the random fluctuation and see the interpolation result. If the result is acceptable, we may try to collect more data to do the stationarity analysis later.

Comments:

Despite what the authors say, however, it is not a model one might use in a situation where cokriging or kriging with external drift were also candidate approaches, since in the case of kriging we assume a linear relationship between z and s, and in KED we assume a linear relationship or a relationship linear in the parameters of some simple fixed effects structure such as a polynomial or (non unduly complex) spline basis. This proposed approach and cokriging/KED would be fundamentally incompatible. Given this, one would expect the authors so start by showing that cokriging or KED are not suitable for these data by plots and exploratory statistics that show (for cokriging) that a linear model of coregionalization is not plausible or (for KED) that no reasonable fixed effects structure looks reasonable.

ANS:

We agree with the Referee#2 that in situations when Cokriging or Kriging with External Drift is applicable, there is no advantage to use the proposed algorithm. However, to satisfy the condition of Cokriging or Kriging with External Drift is not easy to some extent, when it comes to soil moisture content. Following Referee#1's suggestion, we have added some interpolation result of Cokriging in Sect. 4 of the revised manuscript. The new figure (i.e., Fig. 8) indicates that due to the low correlation between soil moisture and spectral variables, the interpolation results of Cokriging are much too smooth, and the details of spatial pattern cannot be reflected. In comparison, the interpolation results of the proposed algorithm seem more alike the reference map (Fig.7). However, the RMSEs of Cokriging and the proposed algorithm are similar. The insignificant differences indicate that the advantage of the proposed algorithm is only in the visual aspect. This is understandable, because we can't expect the new algorithm to outperform the sophisticated Cokriging algorithm in many aspects.

Comments:

Let us assume that the authors do show a sound motivation for applying their model. They must then estimate it appropriately. I think there is a difficulty here. The authors should read the literature on space-time geostatistics to get a better understanding of this. One problem is that, unlike the space-time case, one cannot compute estimates of two marginal variograms (the spatial variogram with the time (or here s) lag =0, and vice versa). In this paper the authors cannot compute a marginal variogram in s space, because it is not possible to find observations with lag 0 in space but some non-zero lag in s. The variogram shown presumably uses lag bins, but the lack of the marginal variogram is a problem because there is likely to be nugget variation in both dimensions and you cannot resolve their contribution to the joint space-s variogram.

ANS:

The referee really has thought deep into the problems of the new proposed algorithm. When we were designing this algorithm, we were handicapped by these problems, too. How to model the variogram in the combined spatial and spectral space? How to separate the variation caused by spatial distance and the variation caused by different spectral signature? And how to give weight to NDVI, albedo and temperature to properly construct the distance in spectral space? There are so many problems to solve. Then, we realized that we should not be trapped by these technical details and should not forget our initial motivation, either, i.e., introducing remote sensing information into the interpolation process, and finally pushing on the practical applications of WSN measurements. Hence, we deliberately chose the simplest solution to these problems and pushed on the overall algorithm. Our intention is, firstly, to introduce the overall ideal and the potential of the proposed algorithm; then the technical problems in the algorithm can be further studied in the future.

By the way, in estimating the variogram model (i.e., Eq. 15), we did not separately estimate the two marginal variograms. Instead, we simultaneously estimated the 3 model parameters (i.e., C0, C1, C2) with surface fitting tools in MATLAB. The problem of “nugget variation in both dimensions” was avoided in this process.

Comments:

There is a second problem. The authors propose a simple model for the space-s variogram which is the sum of a spatial and an s-dimension variogram (Equation 15), but such a model is not in general valid (i.e. it does not define a non-negative definite covariance structure in the overall space). The authors should look at papers such as the one by De Cesare, L., Myers, D.E., Posa, D., 2001.[Estimating and modeling space–time correlation structures. Stat. Probab. Lett. 51, 9–14.] for an account of this and of some valid models.

ANS:

We admit that we have much to learn in geostatistics or space-time correlation structure. The reference paper which the referee recommended is very illuminating to us. However, for the current time, we cannot make too much change to the frame of this manuscript. On the other hand, we do not agree to the referee's judgment that our variogram model (Eq. 15 in the both version) does not define a non-negative definite covariance structure, actually it is non-negative, as is illustrated by Fig. 6.

Comments:

I have a further problem. The authors are using the variogram from a remote sensor for kriging from in-situ sensors. Even if they could be confident that the two variables are measurements of the same underlying quantity the variogram of the remote sensor data is a regularization of the variogram of the networked sensor data onto a very different spatial support. The question of how the remote sensor data might be used to help with this problem is interesting, given the fact that you have some data on the desired support it might be possible to do this, but it requires an explicit change of support step, not just ignoring the difference.

ANS:

Because of the limited number of in-situ sensors, we had to estimate variogram of soil moisture from remote sensing retrieved soil moisture map. The referee pointed out that the spatial support (or footprint) of the in-situ sensors may not be compatible with the support of pixels of the soil moisture map. This is a very good point. As soil moisture is a very heterogeneous variable, the measurement of in-situ sensors may only represent the average soil moisture in less than 1 square meter, but the pixel of the processed soil moisture map is 30*30 square meters to match with the resolution of HJ images. The scale difference between these two datasets is evident and may be an important source of uncertainty.

Although a further investigation into this problem should be conducted, we found it impractical to lay too much stress on this topic in the current frame of paper. So, we only made some discussion of this problem in the conclusion part of the revised manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-401/hess-2016-401-AC3-supplement.pdf

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